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JOSEPH LEIDY, FOUNDER OF VERTEBRATE PALEONTOLOGY IN AMERICA[1]

THE JOSEPH LEIDY LECTURE IN SCIENCE UNDER THE UNIVERSITY OF PENNSYLVANIA FOUNDATION[2]

I ASK the indulgence of the members of this gathering in honor of Joseph Leidy and fellow-workers in the fields of science if I present what I have to say in an informal manner, and I trust that you will not for a moment imagine that because it is presented informally, I do not appreciate the honor conferred upon me in asking me to speak on this historic occasion in reference to a man for whom I have such great admiration as for Joseph Leidy. I shall not repeat except in a very general way the homage that was paid to Leidy in the series of important and penetrating addresses which we have listened to today, but I shall endeavor to present a summary, especially along the lines of paleontology and comparative anatomy, of some of the distinctive features of his work in comparison with those of the men who accompanied and immediately followed him, and to show what great results have come from his efforts as a pioneer and as a founder of this most interesting and fascinating branch of science in America.

Leidy started with an entirely new world of life; he soon learned that he could not base his study of American fossils on the work of Erench paleontologists, for the life of our western regions was not known in the Old World. Every specimen represented a new species or a new genus or a new family, and in some cases a new order. Never was there a greater opportunity than was offered to Leidy in this virgin field of our then virgin West. Never was a man more ready to grasp it than that quiet, unpretentious, unassuming, wonderfully gifted observer of nature. It is particularly interesting to review his work, which was written in the exact spirit of Cuvier, and to see his long record of direct obser-

[1] Extemporaneous address at the Joseph Leidy Centenary, Philadelphia, December 6, 1923.

[2] Two previous addresses have been given in this series: "Heredity and Microscopical Research," by Professor Edward Beecher Wilson, of Columbia University, April 17, 1913; "The Segregation of Genetic Types," by Professor William Bateson, of the John Innes Horticultural Laboratory, January 24, 1922.

vation of the entire extinct fauna not only of the eastern but, especially, of the great western territories. We find to-day how permanent that work was, how little we have to modify it, how well it stands the test of time, how accurate are his descriptions, how perfect his figures and illustrations, and how even to-day they form admirable standards for all the work that has been done since. After a continuous series of epoch-making papers and contributions which he was in the habit of contributing year after year, in meeting after meeting of the academy, he brought his initial work to a climax in 1869 when he published his great monograph, "Extinct mammalian fauna of Nebraska and Dakota." That work still ranks in breadth and accuracy as the finest single contribution that has been made to vertebrate paleontology in this country, if not in the world.

Whereas in Leidy we had a man of the exact observer type, Cope was a man who loved speculation. If Leidy was the natural successor of Cuvier, Cope was the natural successor of Lamarck. Leidy, in his contributions to the academy, covered the whole world of nature, from the protozoa and infusoria up to man, and he lived as the last great naturalist in the world of the old type who was able by both capacity and training to cover the whole field of nature. Cope, in contrast, mastered—and this mastery in itself was a wonderful achievement—the entire domain of vertebrates from the fishes up. Marsh, with less breadth and less ability, nevertheless was a paleontologist of a very high order and had a genius for appreciating what might be called the most important thing in science. He always knew where to explore, where to seek the transition stages, and he never lost the opportunity to point out at the earliest possible moment the most significant fact to be discovered and disseminated.

It is most interesting to contrast the temperament of these three men, Joseph Leidy, Edward Drinker Cope and Othniel Charles Marsh. They were as different as any three men could possibly be made, both by nature and nurture. As Professor Edward Smith said, in one of his addresses on Leidy, "Scientists are only mortals after all." Your scientific genius may hitch up with a star on the one hand and with an anchor on the other. Whereas Leidy was essentially a man of peace, Cope was what might be called a militant paleontologist. Whereas Leidy's motto was peace at any price, Cope's was war whatever it cost. I do not know that I can find from Shakespeare any characterization of Joseph Leidy, but I think in Henry IV there is a pretty good characterization of my friend Edward D. Cope.

I am not yet of Percy's mind, the Hotspur of the north; he that kills me some six or seven dozen of Scots at a breakfast, washes his hands, and says to his wife, "Fie upon this quiet life! I want work." "O my sweet Harry," says she, "how many hast thou killed to-day?" "Give my roan horse a drench," says he; and answers, "Some fourteen," an hour after,—"a trifle, a trifle."

Perhaps there was a scientific providence in all this; perhaps such antagonistic spirits were necessary to enliven and disseminate interest in this branch of science throughout the country. This subtle combative quality in a paleontologist is a strange quality; it is a strange inversion, because the more ancient and difficult the study, the more refractory the fossil, the greater the animation of discussion regarding its relationships. From this subtle ferment there arose the famous rivalry which existed not between Leidy and either of the others, because it was impossible to quarrel with Leidy, but between Cope, the descendant of a Quaker family, and Marsh, the nephew of a great philanthropist. When I took up the subject as a young man and first came to the City of Brotherly Love I always expected to learn of some fresh discussion, some recent combat; it was even in the shade of the Academy of Natural Sciences that one found echoes of these convulsive movements. I remember one day coming into the dignified halls of the academy and finding two of the youthful attendants engaged in hot discussion over a dispute they had overheard at a meeting of the academy the night before.

Leidy, after the characterizations that we have heard of his life from Conklin, Jennings, Scott and others, occupied a pivotal position, a very interesting pivotal position. He was in an intellectual environment and more or less in a social environment entirely different from our own. This is very important to keep in mind in estimating his work. In spirit he was, I think, a true pre-Darwinian in the sense of seeking what may be called facts for Darwin and in the breadth and scope of his researches. But he lived in an entirely different intellectual atmosphere from that which surrounds our scientific world of to-day; he was a John the Baptist for Charles Darwin. We must remember that twelve years before Darwin brought forth the "Origin of Species" this young man was beginning to assemble a mass of data which would have been of great value to the great British naturalist. As shown by Professor Scott, he was tracing the ancestral lineage of the horse, the camel, the rhinoceros, the tapir family, the titanotheres, and last, but not least, the anatomical forbears of man, two matters to which I shall return.

Nevertheless, Leidy was an evolutionist *sub rosa*; he was an evolutionist without ever using the word evolution. There is no doubt about that when you read a citation from his writings such as was selected by Professor Jennings:

> The study of the earth's crust teaches us that very many species of plants and animals became extinct at successive periods, while other races originated to occupy their places. This probably was the result, in many cases, of a change in exterior conditions incompatible with the life of certain species and favorable to the primitive production of others. . . . Living beings did not exist upon the earth prior to their indispensable conditions of action, but wherever these have been brought into operation concomitantly, the former originated. . . . Of the life, present everywhere with its indispensable conditions, and coeval in its origin with them, what was the immediate cause? It could not have existed upon earth prior to its essential conditions; and is it, therefore, the result of these? There appear to be but trifling steps from the oscillating particles of inorganic matter to a Bacterium; from this to a Vibrio, thence to a Monas, and so gradually up to the highest orders of life! The most ancient rocks containing remains of living beings, indicate the contemporaneous existence of the more complex as well as the simplest of organic forms; but, nevertheless, life may have been ushered upon earth, through oceans of the lowest types, long previously to the deposit of the oldest Palaeozoic rocks as known to us!!

This really is a sketch in 1847 of environment and survival such as we now know to be the actual course of evolution and was truly anticipatory of modern results, substituting modern language as we may do for the quaint phraseology of the period.

On the subject of the evolution of man especially Leidy certainly had very clear and positive ideas. He caught from Goethe the significance of the occasional reversion and the embryonic suture between the premaxillary and maxillary bones—constituting a single bone in the human subject, two bones in the lower order of mammals. He pointed out this suture in 1847 in the skull of a native from one of the Hollander Islands. In 1849 he pointed out the separate embryonic condition of the intermaxillary bones. In both cases, as was his habit, Leidy obviously saw the significance but, always sticking to facts and a presentation of facts, he let the matter rest there. The most pronounced adumbration, however, of the evolution of man from the primates is to be found in a citation of his volume of 1873, a period when the descent of man was still not recognized.

> But little change would be necessary to evolve from the jawbone and teeth of *Notharctus* that of the modern monkey. The same condition that would lead to the suppression of a first premolar tooth in continuance would reduce the fangs of the other premolars to a single one. This change with the common teeth shortening and the increase of the depth of the jaw would give the character of the living South American monkey. A further reduction would give rise to the condition of the jaw in the Old World apes and in man.

I do not need to point out that the human jaw, next to the human forehead, is the most significant feature in the transformation from the lower to the higher primates. But some of those here present may not know that a monograph has been written by my successor and colleague Professor William K. Gregory, upon the genus *Notharctus* Leidy. Gregory fifty years after this significant passage was written by Leidy chose *Notharctus* as an ideal intermediate type to place in a theoretic ancestral series leading up to man, and in the beautiful series of preparations which he has recently completed showing the development of the human face in all stages from the most remote ancestral facial type to the modern human face, Gregory uses *Notharctus* as the pivotal point, just as did Leidy fifty years ago.

To return to the matter of Leidy's intellectual environment, how much we owe to-day to our intellectual environment, how much we owe to battles which have been fought and won over insufficient evidence. Not battles of words, but battles of facts. Such evidence as that of *Notharctus* the alert vision of Leidy detected and put in its proper place. In those days "mum" was the word as regards evolution. Neither Cuvier, nor Owen, the British successor of Cuvier, nor Louis Agassiz, great naturalists all, had accepted the theory; theologic influence was still all powerful. Fortunately for Leidy, William Jennings Bryan was still in embryo. Trying to form a historic parallel of William Jennings Bryan, I think it may be found in the figure of King Canute sitting with his court on the shores of Nature, trying to beat back the waves of Truth. If Leidy had lived in the era of Bryan he undoubtedly would have been classified with Professor Conklin and myself—he would have been made with us a type of a new genus, *Anathema maranatha,* in which, according to the zoology of Bryan, are embraced "tall professors coming down out of trees who would push good people not believing in evolution off the sidewalk." Leidy would not have been burned at the stake only because of legal obstacles. Similarly, I think that Professor Conklin and myself owe our lives to the fact that autos da fé in matters of belief are no longer matters of common practice in our civilization!

It is perhaps particularly fitting that Professor Scott and myself were asked to speak at this centenary, for one reason above others. We have been the defendants and supporters of the Leidy tradition. I am not quite sure, but I doubt if you will find in the writings of Professor Cope or Professor Marsh a single allusion to the work of Leidy. I make this statement subject to verification, but I do not recall in their writings a single allusion to the work of Leidy; the rivalry between the two men went to such lengths that in their race with each other Leidy was totally forgotten. Every new animal that was dis-

covered was given a new scientific name by each of them. *Notharctus* Leidy, for example, is exactly the same animal as *Tomitherium* Cope and *Limnotherium* Marsh. Thus arose a trinominal system—three names each for the Eocene and Oligocene animals—the original Leidy name and the Cope and Marsh names. It has been the painful duty of Professor Scott and myself to devote thirty of the best years of our lives trying to straighten out this nomenclatural chaos. Even to this day we are verifying the observations of Leidy; we find that he never made an incorrect observation or published an incorrect figure; his accuracy in these regards is one of his greatest and most permanent claims to immortality as a paleontologist.

I do not know that I altogether agree with my friend Conklin in his address as to the relation of extensive and intensive work. If I understand him aright, he rather implies that intensive work is an inevitable feature of modern scientific progress. I would rather cite Leidy as an example of a man who pursued intensive work and extensive work simultaneously and who had the capacity to pursue intensive work in several branches of science biological and geological, and I would regard the permanence of Leidy's work as largely the result of the state of mind produced by the breadth of his intensive as well as of his extensive work. I would like to leave on your minds my conviction, buttressed by Leidy's life, that it will be necessary even for those of our day to maintain the Leidy attitude, because after all, it is in *the single mind that great hypotheses and theories are generated.* The comparative anatomist, if he dies out, will leave human anatomy impoverished. To-day our students should return to the Leidy attitude, as Professor Scott said, of entering paleontology by way of medicine and base our education in human anatomy as Leidy did on a broad knowledge of comparative anatomy. This is only one instance out of very many that might be given of the legacies of Leidy to us, namely, that throughout his life his mind had continuously the intensive as well as the extensive attitude. He was able to be on the mountain top and then descend into the valleys, and I believe that while some men who pursue one subject intensively all their lives are making great discoveries, for example, such workers as Professor Michelson, whom we all honor, the chances are that few men can make great discoveries unless they approach the subject broadly and work from more than one angle of thought.

Speaking of immortality, I rather share the Leidy view than the view of Cope. I wish it were possible to resurrect Joseph Leidy and to bring him back into the field of modern American paleontology. I wish it were possible to bring him back to life and to have taken him with me, for example, in a motor car across the wastes of Mongolia. I can imagine the joy with which he would have welcomed coming upon the remains of the land dinosaurs, recalling his first description of a dinosaur in America, in the very heart of the great desert of Gobi. And perhaps the still greater joy with which he would have greeted one of his titanotheres, one of the first mammals which he described from Wyoming, out on a great plain on the border of the desert of Gobi.

The desire for this kind of immortality reminds me often of the Greek poet:

> To live like Man and yet like Nature to endure,
> That double gift to Man and Nature both denied
> The Gods alone enjoy.

We are rewriting this beautiful Greek verse in the immortality of Leidy's work and we are holding up his example for the prevailing spirit of truthfulness, which is after all its most characteristic single feature. Would that Leidy and Huxley and Richard Owen and Cuvier and Marsh and Cope could see the heights which have been reached in the branch of science to which they devoted their lives and fortunes. Leidy's infant science, in which it was most hazardous to make predictions, has now reached the stage which I believe is the finest in the history of any science—the stage of prediction—that as astronomers have predicted the existence of unknown and unseen planets, paleontologists can also predict unknown and unseen forms of life and, moreover, can point out where they may be found.

Is our paleontological path reaching its goal? I think not. Its final goal will be reached when paleontologists are able through extensive and intensive methods to join hands with workers in other biological fields and when we are able, pursuing our branch in the Leidy spirit, to bring together into one harmony, the harmony which certainly exists, although at present we do not see it, by bringing together into one harmony the great underlying principle, the multiple aspects of which we can sum up in the word evolution.

Henry Fairfield Osborn

American Museum
of Natural History

A GENETIC VIEW OF SEX EXPRESSION IN THE FLOWERING PLANTS[1]

It seems a conservative statement to say that stud-

[1] Address of the president of the American Society of Naturalists, forty-first annual meeting, Cincinnati, December 29, 1923.

Paper No. 117, Department of Plant Breeding, Cornell University, Ithaca, New York.

ies of the past twenty years among animal forms have tended increasingly to link the phenomena of sex inheritance with the behavior of chromosomes. To this result, cytology and genetics have contributed perhaps almost equally. The number of forms in which one sex is known to have a morphologically different chromosome complex from the other sex are many. That, with respect to the chromosomes, the female of certain forms produces gametes of a single kind, whereas the male produces two kinds, and that in turn an egg fertilized by one kind of sperm gives rise to a female and with the other kind to a male, cytological studies leave no doubt. In other forms it is the female that produces two kinds of gametes and the male one kind. The fact that in some animals sex dimorphism is associated with unequal numbers of chromosomes while in others, though the numbers are the same, the sex chromosomes differ morphologically in the two sexes, makes it seem not unlikely that functional dimorphism may exist even where no morphological differences in the chromosomes are seen.

No less important than these cytological discoveries and quite in accord with them are the results of genetic studies of sex-linked characters. The exact parallelism between genetic phenomena and chromosome behavior as normally exhibited, for example, in criss-cross inheritance of sex-linked characters is no more striking than that shown in aberrant cases involving primary and secondary non-disjunction, the occurrence of gynandromorphs, and the like. In fact, the unity of the results obtained by cytological and genetic methods of attack may well be regarded as among the most brilliant achievements of recent biological research.

Letting these statements represent the present trend of research on the animal side and, for the moment, omitting any reference to results that are interpreted on quite a different basis, we may now inquire into the present status of the sex problem among the higher plants. Here, it must be confessed, there is found a very different situation. If among zoologists there are still some whose results lead them to dissent more or less mildly from the current chromosome theory of sex inheritance, among botanists there are crusaders on whose banners are inscribed a quite different device. In making this statement, I do not overlook the fact that some botanists have made bold to suggest a Mendelian interpretation of sex inheritance in dioecious plants, regarding one sex as a homozygous recessive and the other as a heterozygous dominant. But it will scarcely be denied, I think, that the present trend of botanical thought is strongly counter to any current chromosome theory of sex inheritance.

Indeed there are botanists who apparently are not convinced that there is any relation between chromosomes and the genetic factors concerned in the development of even such characters as color of seeds and flowers or the numerous other qualities which are the stock in trade of geneticists. In fact, there are botanists—there may be zoologists too for all I know—who, I am told, are not at all favorably disposed toward the notions of geneticists that there really exist as entities such things as genetic factors, botanists whose rallying cry, it is said, is "Down with the gene." I have used the expressions "I am told" and "it is said" because I confess that I can not quite follow the published statements of these authors. To them I am not now addressing my remarks. Evidently, we neither read nor speak the same language.

But there are other botanists who accept in whole, or in large part, the orthodox genetic faith for ordinary Mendelian characters, even to the linear arrangement of genes on the chromosomes, and who, none the less, will have nothing to do with hypotheses that in any way connect chromosomes with sex even in dioecious plants. They seem to regard sex and sex characters as wholly different from other plant characters both in their inheritance and in their expression. To them sex development is in no way conditioned by genetic factors.

Never before have I admitted having in my own genotype much of the missionary spirit, but now I must confess to an inclination to convert the class of botanists I have just alluded to. Surely their souls are worth saving.

You doubtless will have gathered from all this that my thesis is that sex characters differ in no essential way from other organic characters, as regards either mode of inheritance or manner of development. In defending this thesis, it will be necessary to inquire why one might possibly suppose that sex characters are essentially different from other characters of plants or animals.

Perhaps a prime consideration in forcing one to question seriously whether sex in the higher plants is influenced by genetic factors in any way related to the chromosomes is the prevalence of the hermaphroditic condition among these organisms. Another difficulty, one more apparent than real, is the obvious complexity of sex differentiations in contrast to the supposedly simple conception of sex chromosomes. Again, how can one account for the environmental modification of sex characters or the outright reversal of sex? Whether or not such effects of environment have actually been proved for animals, it is waste of time even to raise the question for plants. We must begin by admitting that, in at least some plants, sex expression is reversible.

Complexity of Sex Expression

Let us consider first the manifest complexity of sex

expression in the flowering plants.[2] I need give no extended account of this. The diverse forms and arrangements of stamens and pistils are familiar to all. Starting with types in which both stamens and pistils are found in the same flower, we pass, by no very ab- and pistillate flowers in the same inflorescence or rupt stages, through monoecious types with staminate separated widely in distinct inflorescences, to dioecious forms, not overlooking along the way various combination types exhibiting andro- and gynomonoecism and andro- and gynodioecism. How can such complexity be harmonized with the conception of simple unit factors as expounded by geneticists? If we grant that genetic factors do exist and that they do play their part, even though in an unknown way, in the development of ordinary characters such as form, size or color of seeds or other plant parts—and please recall that I am addressing my remarks only to those who do accept some part of all this—it may be profitable to inquire whether these ordinary characters are in reality so very simple in their manner of inheritance.

The notion that some or all of these characters are inherited in a very simple way is, I fear, largely the fault of geneticists, an error that was excusable perhaps in the early stages of our studies. When, to use an example with which I am personally familiar, it was found that a single genetic factor pair in maize differentiates normal green seedlings from ones wholly devoid of green color, it doubtless was correct to conclude that a single recessive factor is sufficient to prevent the development of chlorophyll. Even at that time, however, it was going much too far to infer as a corollary to this that the dominant allelomorph of this factor for white seedlings is alone concerned in the normal development and distribution of chlorophyll. No wonder physiologists were unable to accept so simple an explanation of the inheritance of so complex a substance as chlorophyll. We now know at least four distinct recessive factors, any one of which assures the production of white seedlings, and others which accomplish this end only when acting together. And there are other factors for virescent, pale green, yellow and striped seedlings, and still others for various kinds of abnormal development of chlorophyll in older plants. In all, there are known more than thirty recessive genetic factors, any one of which is able to retard or to prevent the normal development of chlorophyll. From this it must follow that all the thirty or more dominant allelomorphs of these recessive factors are essential to the normal development of chlorophyll in the maize plant. This should be complex enough even for a physiologist. And we have only begun the genetic investigation of chlorophyll inheritance in maize.

Although this may be an extreme example, it is more or less typical of other common characters. If we grant that the complexity of the situation here is no bar to a factorial interpretation, why worry about the complexity of sex characters? But, is not the chromosome theory of sex inheritance a relatively very simple thing? What is more simple, for instance, than the conception that two X chromosomes in Drosophila make a female and one X a male?

Let us see just how simple the sex situation in Drosophila is not. It was realized long ago that the case could not be explained by the assumption that there was merely a factor for femaleness in one or both of the X chromosomes of the female and a factor for maleness in the X chromosome of the male, for normally the X chromosome of every male is derived directly from its mother. Although explanations of this situation were not wanting, they were none too plausible. There might perhaps be a maleness factor in the Y chromosome, but then how account for sex dimorphism in forms lacking the Y chromosome?

It was not until individuals with triploid autosomes were found that the situation began to clear. It seems highly probable now that the X chromosomes of Drosophila carry female tendencies—perhaps male ones also, but with the balance on the female side—and that the autosomes, or some of them, carry a balance toward the male side. With diploid autosomes, two X chromosomes throw the balance strongly to the female side, whereas one X chromosome is insufficient to accomplish this result and the maleness of the autosomes completely overbalances it. It is easy to believe, then, that the several possible combinations of haploid, diploid and triploid chromosomes might well result in supermales, males, sex intergrades, females and superfemales, all of which have been observed in Drosophila and some of which have been noted in other forms. The usual sharp distinction between males and females in Drosophila is apparently due to the fact that normally the displacement of a single X chromosome is enough to throw the balance from one sex tendency completely to the opposite tendency.

Granting the probability of all this, is not sex inheritance still very different from the inheritance of other characters? Other characters of Drosophila are

[2] Although many may not sanction my use of the term sex as applicable to what we commonly regard as the plant body, the sporophyte, of flowering plants instead of limiting its application to what phylogenetically is the sexual generation, the gametophyte, they will certainly admit that there is precedent for this usage and perhaps also that there is some real justification for it both on the basis of convenience in comparing the higher plants with the higher animals and on the more fundamental ground that in the flowering plants sex differentiation of the all but vestigial gametophyte is anticipated in the sporophyte.

referred to specific genes assigned definite *loci* on the chromosomes. This has never been done with sex genes. Must we conclude, therefore, that sex is not dependent on specific genes but that there are merely somewhat different sex tendencies among the several chromosomes, a kind of "organism-as-a-whole" conception narrowed down to the chromosomes? This conclusion does not necessarily follow, even though at present we may have no direct evidence against it. There might be a hundred distinct sex factors in Drosophila without the possibility of assigning a single one of them to a definite *locus* so long as they were all in the homozygous condition. The same thing was true of the allelomorphs of some hundreds of mutant genes for other characters of Drosophila before the mutations occurred or the mutants were discovered, and there are perhaps hundreds of other genes that remain unplaced and unknown merely because they have not mutated or the mutants have not been studied.

The genetic situation in maize, though on the whole much less well known than that in Drosophila, may perhaps afford some help toward a solution of sex problems. Normal maize plants are monoecious, with maleness expressed in the terminal and femaleness in the lateral inflorescences. There are, however, mutant types of maize which, under ordinary conditions, are wholly female, the terminal as well as the lateral inflorescence bearing pistillate flowers only. Four distinct recessive genes are known, the influence of any one of which results in femaleness. Moreover, two of these have been definitely placed with respect to genes for such characters as color of seeds, color and form of leaves, and the like, and their *loci* are in nonhomologous chromosomes. Although no entirely male strains of maize are known, there are three types which ordinarily produce only a few pistillate flowers and some individuals of which have no such flowers. And each of these again is due to one or more recessive factors, each one being transmitted independently of the others. Moreover, there are two types in which the normal monoecious condition has been modified to an andromonoecious one; and each of these two types also is due to the influence of one or other of two recessive genes. In all, then, there are at least nine pairs of genetic factors which influence the expression of sex in maize.

It is not yet known whether all these nine pairs of genetic factors are to be assigned to nine of the ten pairs of chromosomes, but certainly several of them are inherited independently. Although no dioecious strain of maize is known to exist and the sex situation in this plant may not be closely similar in some other respects to that in Drosophila, it none the less adds to the plausibility of the present interpretation of sex, namely, that sex is probably an expression of the interaction of several, perhaps many, factors located in different chromosomes. In any event sex inheritance is not the simple affair that it has been supposed to be. In this respect, sex inheritance corresponds closely to the hereditary behavior of other characters.

Sex Reversal

We may now consider the problem of sex reversal particularly in dioecious plants. Numerous instances are known of the appearance of a few or many staminate flowers late in the life of plants that earlier had produced none but pistillate flowers, or of plants in which this behavior is reversed. Some monoecious plants usually first have staminate flowers alone, then both staminate and pistillate flowers, and finally, under certain environmental conditions, only pistillate ones. Plants of one sex, which under ordinary conditions do not usually produce flowers of the opposite sex, can often be made to do so by appropriate cultural conditions. I have referred to a wholly pistillate flowered condition of maize as being dependent on the presence of one or other of certain recessive genetic factors. Normal strains of monoecious maize can be so grown that they produce no staminate flowers and are then indistinguishable in appearance from the "genetically" pistillate flowered kinds.

Does this behavior set off sex inheritance and sex development in any way from the inheritance of other characters? The behavior of numerous vegetative characters answers this question in the negative. A single extreme example may be noted. In some strains of maize a red pigment develops in the pericarp if the ear is exposed to light at the proper time and no such color is produced if the ear is not so exposed. Long exposure to strong light results in strong color whereas shorter exposure or weaker light gives correspondingly weaker color. Is this then a matter of environmental influence alone with which genetic factors have nothing to do? Obviously not, for there are other strains of maize whose ears have never been observed to develop such color under any condition of light. Again there are strains that have red color in the pericarp whether the ears are exposed to light or kept in darkness. The genetic factors concerned in the expression of color in these several strains are well known and their *loci* in the chromosome complex of maize have been determined. In short, the development of this so-called sun-red color in the pericarp of maize is just as much a genetic phenomenon as that of any other character of this plant. Merely because its expression is influenced more by environment than is true of some other characters, it does not follow that the genetic contribution is any less real or any less important.

I should not dare affirm that there is any character

whose expression is not at all influenced by environment and I confess to an abiding faith that characters of all sorts are influenced by genetic factors. I assume—and admit that my position is an assumption—that characters in general, whether of sex or of other nature, develop through the cooperative influences of genetic factors and of factors of the environment, the internal as well as the external environment. I say cooperative influence because I see no need to assume antagonistic effects of heredity and environment. While it is conceivable that a favorable environment may force the development of a character beyond the normal expression of its inheritance and that an unfavorable one may stop its development short of its inherent possibilities, is not such a conception a bit absurd? Just what is inherited? Is not after all what is inherited merely the possibility—indeed the necessity—of reacting in a particular way to a particular internal and external environment? I never think of sun-red maize as inheriting red pericarp and of a certain environment, darkness, inhibiting the full expression of this inheritance. Nor do I think of it as inheriting a colorless pericarp which a particular environment, sunlight, changes to red. To me it inherits merely the ability to react to sunlight so as to produce red color and to darkness in such a way that the end result is colorless pericarp. And other strains of maize inherit the ability to react to these same environments in quite different ways as respects pericarp color.

You now have my creed of inheritance and development. True, we know very little of how these reactions begin or of what they are. Do we know much less about these processes when they concern sex development than when vegetative characters are in question? All I care to risk saying is that in many animals, and perhaps in some dioecious plants, the balance of genetic factors is so strongly toward maleness or toward femaleness that the reactions give a definite result in any environment as yet tried, whereas in many dioecious plants, and perhaps in some animals, the genetic balance is so delicate that the reaction may go one way in one environment and the other way under other surrounding conditions, with the occurrence of various sex intergrades when the environment is less extreme or less constant. By substituting pericarp color in maize for sex in animals and plants, the foregoing statement need not be otherwise changed to make it fit the observed results. In short, we have as yet come upon no fundamental difference between the inheritance of sex and the inheritance of other characters.

HERMAPHRODITISM[3]

There remains the difficulty of accounting for the hermaphroditic condition so prevalent among the higher plants. How can hermaphroditism be reconciled with the idea of definite genetic factors for sex? The staminate and pistillate flowers of a monoecious plant differ from each other as sharply in form and function as do these two kinds of flowers on related dioecious plants, and there is the same sharp difference between stamens and pistils when they occur together in the same flower as when separated in different flowers or on different plants. Is there the slightest cytological evidence of the existence of any chromosome mechanism which could conceivably distribute allelomorphic sex factors to the different parts of a single plant in somewhat the same way that the reduction division may do in case of dioecious plants?

If you have followed me thus far, you doubtless have anticipated my treatment of this problem. It is idle in the face of the negative evidence of cytological studies to postulate any chromosome behavior, analogous to the reduction division, for the distribution of sex difference to different parts of the same plant. Moreover, I am aware of no critical genetic evidence in support of the idea that unit factors are ever, or at all commonly, separated in the sporophyte body in any way analogous to Mendelian segregation. On the contrary, there is strong genetic evidence that bud sports and related phenomena, often ascribed to segregation of factors, are due at least in some instances to chromosome elimination or non-disjunction or to somatic gene mutations.

If then there is neither cytological nor genetic evidence of the segregation of unit factors within the plant body, how are we to account for sex differentiation in hermaphroditic plants? Again I answer that the explanation is to be sought in the same way that an understanding of the differentiation of other characters will, let us hope, some day be gained. I grant that it is not an explanation of sex differences to say that the fundamental processes concerned in the differentiation of other characters are little known. But I do believe that it will clarify our ideas of sex differentiation to get away from the notion that it is necessarily different in any essential way from the differentiation of other characters.

It seems unreasonable to suppose that sex in hermaphroditic plants can in no way be related to sex genes merely because there presumably is the same gene complex in the cells of a stamen that there is in the cells of a pistil of the same plant, the same gene complex in a microsporocyte as in a megasporocyte

[3] Following zoological precedent, I here use the term hermaphroditism with its common-language meaning to denote the expression of both maleness and femaleness in the same individual, rather than with the restricted significance given it by botanists to designate individuals or species in which each flower is bisexual. Botanists may, if they choose, with little or no violence to my meaning, substitute for it here the term bisporangeateness.

of the same individual. This is also true doubtless of other plant parts. Aleurone color of maize is seen in only one or two layers of cells, the outer layers of the endosperm. Just what there is in the internal or external environment of these cell layers that induces the color to develop in them, while no such color is ever seen in the underlying cells of the endosperm, is not known. It does not follow from this, however, that genetic factors are not concerned in aleurone-color development. Indeed, the genetic complex concerned with aleurone color is as well understood as any in maize. Several distinct aleurone-color genes have long been recognized and the relative positions of their *loci* in the chromosomes are in large part known. I see no reason to doubt that the same complex, the same relative position of genes, holds for all cells of the endosperm, but the deeper lying cells do not exhibit the colors seen in the aleurone layer. Is the association of pistils and stamens in a single flower either more or less mysterious than the differentiation of the aleurone layer from the rest of the endosperm?

The aleurone-color comparison can be carried still further. Certain genes are known to influence aleurone-color patterns. In the speckled and in the blotched patterns, smaller or larger groups of aleurone cells develop color while neighboring groups do not. In the Navajo pattern the distal end of the seed is solidly colored and the remainder colorless. No one, I presume, would suggest that in these cases certain aleurone cells have a different complex of aleurone-color genes from other nearby aleurone cells.

If the aleurone-color situation is open thus to factorial analysis, need we despair of accomplishing as much for sex differences even in hermaphroditic plants? True, little along this line has been accomplished as yet, but it is worth recalling that a beginning has been made with maize. It was noted earlier that any one of nine recessive factors is sufficient even under ordinary conditions of growth to upset the typical monoecious habit. It follows from this that the presence of all nine of the dominant allelomorphs of these factors is necessary for the expression of the typical monoecious condition. True, it is not known how this complex of nine dominant genes, with perhaps many others not as yet recognized, results in the production of male flowers in the terminal inflorescences and female flowers in the lateral ones; but do we know less about this than about how a particular complex of endosperm and aleurone genes results in definite color patterns?

Summary and Conclusions

In concluding this discussion let us recall that, according to the view here presented, sex characters of the flowering plants are to be interpreted on the basis of genetic factors associated with chromosomes just as vegetative characters are interpreted. In typically hermaphroditic forms where both male and female tendencies are exhibited in the same individual, factors for maleness and for femaleness are thought of as being in so delicate a balance that influences analogous to those responsible for the differentiation of vegetative characters effect a more or less regular differentiation of sex organs. The question of heterozygosity of factors for one sex or the other does not enter here any more than it does in cases of inherited color patterns where pigmented and unpigmented parts may appear in some forms as irregular mosaics and in others as regular patterns. Genetic factors influencing the development of male and female organs in the monoecious species, *Zea mays*, have been shown to behave quite as do other genes.

In prevailingly dioecious forms of the flowering plants also, factors for maleness and for femaleness presumably are present both in male and in female individuals, but here the balance is more strongly to the one or to the other condition. The approximate numerical equality of individuals of the two sexes in these forms at once suggests a chromosome mechanism similar to that known to exist in numerous animal groups. The occurrence of sex linkage in a dioecious species of Lychnis favors this assumption, although certain irregularities of behavior in this instance are still to be explained. There has appeared recently a preliminary cytological account of chromosome dimorphism of the X–Y type in another species of Lychnis. That morphologically unlike sex chromosomes have not been found, and may not exist, in many plants need not disturb us greatly, for why should sex factors be presumed necessarily to influence the size or form of chromosomes more than do other genes? The suggestion that the difference frequently observed between the X and the Y chromosomes of many dioecious animals may be due to the opportunity for the indefinite accumulation of recessive zygotic lethals, which is afforded by the enforced heterozygosity of one sex, appeals to me strongly. If these lethals were at all frequently of the nature of chromosome deficiencies, it is conceivable that they might modify profoundly the form and size of the Y chromosome. I see no reason, however, to assume that lethals of this kind occur in all dioecious organisms. Moreover, it seems likely that the occasional self-fertilization of prevailingly dioecious individuals among flowering plants may tend to prevent the accumulation of recessive lethals in the heterozygous sex. But there is little use in discussing such possibilities until we have much more information about the genetic behavior of dioecious plants than is now available.

The existence of sex intergrades is no bar to the conception of sex relations in dioecious plants here outlined. The distinctness of predominantly male and predominantly female individuals ordinarily is marked and may well be ascribed to a pair of differential genes distributed with homologous chromosomes at the reduction division. The difference between this and instances of absolute dioecism, if such exist, is to be sought in the nature of the respective genes rather than in chromosome behavior. Even the occasional appearance of sex intergrades approaching the condition of typical hermaphrodites may well be due to the influence of several heterozygous sex factors of relatively minor influence—the geneticist's old friends, modifying factors, in a somewhat unfamiliar rôle.

I have reserved for this, the position of emphasis at the close of my discussion, the strongest evidence against my view of sex relations in dioecious plants. When, as not uncommonly happens, an otherwise female plant produces a few male flowers or a male plant a few female flowers, it is possible to obtain self-fertilized seeds. If, then, one sex is heterozygous for a strongly differential pair of sex factors and the other sex is homozygous for the recessive allelomorph, the latter should, of course, breed true and the former presumably throw the two types in the numerical relation typical of a Mendelian monohybrid. Results reported for at least one form, Mercurialis, indicate that individuals exhibiting a predominantly female condition breed true when self-fertilized. But there is no evidence, so far as I am aware, that predominantly male plants of this form throw the two types. Indeed, the available evidence is quite the opposite of this.

Unfortunately, Mercurialis is not well adapted to an investigation of this kind. When relatively few female flowers are produced by a male plant and such flowers produce only a few seeds, the number of plants resulting is correspondingly small. Perhaps, however, the numbers actually reported for Mercurialis are sufficient to carry conviction to one who does not have preconceived notions contrary to the observed results. The results with Mercurialis, as well as the striking departure from normal sex ratios in the progenies of certain individuals of Lychnis, emphasize the importance of thoroughgoing analyses of similar material to the end that such possibilities as the presence of differential gametic lethals, and the like, may be checked.

It is just here that one finds Mercurialis, and in fact most dioecious plants, unsatisfactory material at the present time. The genetic complex of none of these forms is at all well known. If for some dioecious species of plant we could know the chromosome *loci* of numerous genes, we should hold a much more favorable position than at present from which to attempt an analysis of its sex behavior. I am not suggesting that we wait until such material is available, but I am not optimistic about the possibility of obtaining crucial evidence from any species until its genetic analysis has proceeded to a point that makes available the tools essential to any critical genetic investigation of its sex expression.

Finally, let me observe that, even though this missionary epistle to the brethren who dwell in darkness fail to convert them, it should at least afford them a somewhat unfamiliar point of attack. And, if their subsequent efforts result in my own conversion, I, at least, shall feel that I have not labored in vain.

R. A. EMERSON

CORNELL UNIVERSITY

REORGANIZATION OF THE NAPLES ZOOLOGICAL STATION

AMERICAN biologists will rejoice in the good news that the Zoological Station of Naples is in course of reorganization, with Dr. Reinhard Dohrn as its executive head. We owe a large debt to the station. An opportunity now offers to make some repayment by helping in a process of reconstruction that will be of far reaching importance for biological research in this country. Detailed information recently received from several sources, including Dr. Dohrn himself, makes it seem desirable to direct attention to the following facts.

Under the direction of Anton Dohrn from 1875 to 1909, and of his son Reinhard from 1909 to 1914, the station held an undisputed position as the leading internationl center of biological research in the world. It was originally founded and equipped with funds derived from the private fortune of Anton Dohrn and of his personal friends (including about 25,000 francs from English friends, among whom were Darwin, Huxley and Francis Balfour). These funds were supplemented at various times by other contributions, including 50,000 francs from the Berlin Academy of Sciences, 100,000 francs from the Italian government (for the first enlargement of the station), 400,000 francs from private subscriptions in Germany, England and the United States (for second enlargement) and about 100,000 francs raised by a subscription started by the International Zoological Congress in 1910 for renewal of the exploration steamers. The city of Naples generously provided a site in the public garden for a building to be constructed at Dohrn's expense, a contract being signed in 1875, and extended in 1894 and 1903, under which the station and its contents was to become the property of the City of Naples in 1965. Under this arrangement the station was established as an essentially private en-

terprise, with Dohrn, and after him his sons, as its legal proprietors.

The income of the station for costs of operation was derived from various sources. These included entrance fees to the splendid aquarium and the sale of preserved material; yearly subventions of 5,000 francs from the Italian government, and of sums varying from 25,000 to 50,000 francs from the German empire; and, above all, yearly subscriptions for investigators' tables from various governments, universities, scientific academies and other bodies, amounting to 2,500 francs per year for each table. The use of these tables brought to the station biological investigators from nearly every country in the civilized world; and Dohrn's broad-minded and generous attitude towards all comers, without regard to distinctions of nationality, gave to the station an international spirit of friendly cooperation and scientific fellowship that formed one of the most powerful factors in the long continued success of the enterprise.

Before the war about fifty investigators' tables were regularly rented, distributed as follows: Germany 12, Italy 11, United States 5, Russia 4, England 3, Austria 3, Belgium 2, Holland 2, and one each from Hungary, Switzerland, Rumania and Japan, besides two from the City and Province of Naples (these do not include the abovementioned subventions from the Italian and German governments). Upon the entrance of Italy into the war a great change took place. Reinhard Dohrn, the director, and other foreign members of the staff were obliged to relinquish the administration of the station, which was then placed in the hands of a commission of three members appointed by the Italian government. During the war nearly all of the foreign table subscriptions lapsed (including the five from the United States), only the British three being retained; and although the station was nominally kept open, its work was practically at a standstill.

At the end of the war strong efforts, supported especially by Benedetto Croce, the minister of education, were made to restore the *status quo ante*, including the complete reinstatement of Dr. Dohrn. Such action was actually taken in 1920 by royal decree; but this was contested in the courts, and after a period of litigation the situation was greatly modified by renewed governmental action. The outcome of all this has been the recent establishment of the station on a new basis by governmental decree, as a special form of autonomous public corporation (*"ente morale"*) the control of which is vested in a board or "Administrative Council," composed of the mayor of Naples and six other members to be appointed every five years by the minister of education. Dr. Dohrn, in addition to membership in this council, is to be appointed director and administrative head of the station. It is the intention both of the Italian authorities and of Dr. Dohrn to preserve as far as possible the international character of the station and to carry forward its work along the lines laid down by its distinguished founder and adhered to with such brilliant results for more than forty years.

Dr. Dohrn is anxious to reestablish the cooperation between the station and the scientific institutions of other countries; and all who have the advancement of biological science at heart will cordially share in this desire. Before the war the United States was represented at Naples by five tables, two subscribed for by the Carnegie Institution of Washington, one by the Smithsonian Institution, one by an association of public-spirited American women, and one by Columbia University. We believe that the time has come when this country should join with others in renewing its support of the station and in upholding the policy of its director in every possible way. No one is so well fitted as Reinhard Dohrn to keep alive the ideals of Anton Dohrn and to perpetuate the traditions of international scientific fellowship that he upheld in so large and generous a spirit. And we believe that cooperation with him towards that end will be in a line of accomplishment which, in the existing state of civilization, means more by far than the advancement of science in any narrowly technical sense.

Edmund B. Wilson

Columbia University

SAMUEL PHILIP SADTLER

Samuel Philip Sadtler was born in Pine Grove, Pa., July 18, 1847, and died in Philadelphia, December 20, 1923. His father was a Lutheran minister and he received his early education in various communities, wherever his father happened to be stationed. He was graduated from the Easton (Pa.) high school in 1862, and from Pennsylvania College, Gettysburg, Pa., in 1867. He took a year of instruction at the new Lehigh University, Bethlehem, Pa., and then went to Harvard, where he did advanced work under Dr. Wolcott Gibbs, graduating with the degree of B.Sc. from Lawrence Scientific School in 1870. He then went to Göttingen, Germany, where he studied under the famous Professor Wöhler, earning the degree of Doctor of Philosophy, which was granted him in 1871.

Returning to America he accepted the professorship of chemistry and physics in Pennsylvania College and served three years, when he removed to Philadelphia to take the chair of general and organic chemistry in the University of Pennsylvania, which position he held until 1891. In 1878 he helped Dr. Robert Bridges with his lecture work at the Philadel-

phia College of Pharmacy, and the following year succeeded him as professor of chemistry, holding the chair for 37 years, or until 1916, at which time he retired from active teaching work and was elected Emeritus Professor of Chemistry.

He was the author of a "Handbook of chemical experimentation for lecturers and teachers"; the first American editor of Attfield's "Chemistry"; the author of "Industrial organic chemistry" (Sadtler); and, with Professor Henry Trimble, author of "Pharmaceutical and medical chemistry." In addition, he contributed freely to the columns of scientific publications of a periodical character.

For a quarter of a century he was chemical editor of the United States Dispensatory; thrice he was a delegate to the United States Pharmacopoeia convention and for two decades was a member of the Revision Committee of the Pharmacopoeia.

He was a member of a large number of scientific organizations, having been one of the founders and the first president of the American Institute of Chemical Engineers.

In addition to his professional activities he maintained a laboratory for commercial work and, because of his wide knowledge of industrial processes, was frequently retained by one of the parties concerned in litigation over alleged infringement of patent.

He was active in the work of the Lutheran Church and served many years on its National Board of Publication. For many years he was a member of the board of trustees of the Philadelphia College of Pharmacy, and served as its chairman during the three years preceding his death. During the 40 years of his connection with the College of Pharmacy he was very active in the conduct of its affairs in many ways, and not a little of the prestige it has in its field of work to-day is due to his labors, his judgment and his wise counsel.

As a man among men he was ever kind and courteous to the "other fellow," no matter how much he might differ from him in thought or character, even-tempered, clean of speech, respected by all who knew him at all, loved by those who knew him well, a fine example of a cultured Christian gentleman.

CHARLES H. LAWALL

PHILADELPHIA COLLEGE
OF PHARMACY

SCIENTIFIC EVENTS

THE ROTHAMSTED EXPERIMENTAL STATION AND UNIVERSITY DEGREES

THE University of Cambridge is prepared to give favorable consideration to each individual case of applicants who desire to carry out at Rothamsted a portion of their work for the degrees of M. Sc. and Ph.D.

The University of London has accepted the Rothamsted Experimental Station as a recognized institution from which research workers may submit work done at Rothamsted for the degrees of M.Sc., Ph.D. and D.Sc.

Intending workers at Rothamsted are strongly advised in the first instance to send a full account of their academic qualifications and training to the director, as the candidate will be allowed to enter his thesis only if these qualifications are acceptable to the university senate.

The general conditions imposed by the university regulations are briefly:

(1) *Cambridge.* These degrees are granted in full to men only; under certain limitations the titles of degrees are open to women without the privileges which the degree confers in the university. *M.Sc.* A minimum residence of five terms at Cambridge and one at Rothamsted. Thesis to be presented not earlier than the sixth and not later than the twelfth term from the term of admission as a research student. *Ph.D.* A minimum residence of six terms at Cambridge and three at Rothamsted. Thesis normally to be presented not earlier than the ninth term and not later than the twelfth term from term of admission as a research student.

(2) *London.* These degrees are open to men and women on equal terms. *M.Sc. and Ph.D.* A minimum residence of two calendar years at Rothamsted before submission of the thesis. *D.Sc.* Normally the candidate must first hold the M.Sc. degree of the university, but in special cases, on the ground of published work, this regulation may on application be waived. A residence of two years at Rothamsted is required. A student must ordinarily have taken his first degree not less than four years before the date of his entry for the D.Sc. examination. In the case of workers already holding a first degree of London University, they may enter as external students for higher degrees without any requirements as to residence.

A WORLD LIST OF PERIODICALS[1]

A VALUABLE piece of bibliographical work which should simplify the task of science is now well on the way to completion. The results of researches, before they can be used by others than the original investigators, have to be printed and published. Their form is technical and their appeal is not popular, so, for the most part, they have to seek special vehicles of publication. Every civilized country supports a number of periodicals devoted to such work. In many

[1] From the *London Times*.

cases they are issued by learned societies or by universities; in others by semi-private groups of scientific persons. Few have a wide circulation and most appear rather irregularly. Some even have brief existences or change their names and places of issue; and yet in any one of them at any time there may be work of vital moment to other workers in the advancement of knowledge, or ready for immediate application to human needs.

The Conjoint Board of Scientific Societies, before it was dissolved, more than a year ago worked out a scheme for preparing and publishing a list of the scientific periodicals published all over the world within the years 1900–1921, with an indication of the chief centers in Great Britain and Ireland where any of these periodicals were taken. The board, before winding up, transferred the scheme to Sir Arthur Schuster, Mr. Robert Mond and Dr. Chalmers Mitchell as trustees to place it on a permanent footing. This has now been done by the formation of a non-profit sharing registered company, "The World List." A large number of libraries have promised to subscribe for the volume when issued and the Carnegie United Kingdom Trust have generously guaranteed a grant-in-aid by which publication is assured.

The trustees of the British Museum, recognizing the importance of the undertaking, consented to allow the work of compilation to be made part of the official duty of the Department of Printed Books. Under the able direction of Dr. A. W. Pollard, keeper of the department, the compilation of the list has now been completed. Over 20,000 scientific periodicals have been catalogued and arranged in alphabetical order, but it is already apparent that before the volume is finally printed materials will have accumulated for a supplement containing certain journals whose existence had not been discovered, as well as others published since 1920.

The Clarendon Press has undertaken the printing. The first sheet has already been passed through the press, and the work will go on continuously until completion. The lists are in double columns on quarto pages printed only on one side of the page, so that there will be space for such notes as to the contents or character of the periodicals as may suit individual libraries to make. By the use of a rubber stamp to indicate which periodicals it contains, any library may turn this part of the volume into its own catalogue.

So far the compilation has required only time and expert knowledge. There now remains the difficult but vital business of indicating the chief libraries at which the periodicals may be found. In the alphabetical list each entry has a consecutive number. It will be followed by an index-section, in which the number assigned to each periodical will have printed against it alphabetical symbols for the cooperating libraries possessing a file of it, printed in small capitals grouped under alphabetical symbols for the towns in which these libraries are situated. A third section will explain the symbols for the towns and libraries.

THE LIBRARY OF THE NEW YORK BOTANICAL GARDEN

CONSIDERABLE progress has been made in classifying and shelving the newly acquired collection of books purchased for the Library of the New York Botanical Garden from Geneva, but the work, which is being pushed as rapidly as possible by Dr. John Hendley Barnhart, bibliographer, and Miss Sarah H. Barlow, librarian, will require considerable time to finish. The Geneva collection when received filled ninety-three large packing cases and weighed more than twelve tons.

A brief summary of the collection, which was sold for 72,000 Swiss francs, and which is considered the most important collection of books on botany and horticulture that has come to the new world from the old, has been made to the *Times* by Dr. Barnhart. He is reported to have said that the city of Geneva, Switzerland, has long been the home of three botanical institutions ranking among the most famous of their kind in the world. One was the De Candolle herbarium, established by Augustin Pierre de Candolle (1778–1841), and maintained and developed by his son Alphonse, his grandson Casimir, and his great grandson Augustin de Candolle. Another was the Boissier herbarium, established by Pierre Edmon Boissier (1810–1885), afterward owned and enlarged by his son-in-law, Willian Barbey. Until ten years ago these two great collections remained under private ownership, although visited and used almost as if they were public institutions by students from all parts of the world. The third, actually public, was the botanical conservatory of the city of Geneva, its great collections brought together from various sources, but based originally upon the Delessert herbarium, one of the most extensive private collections of dried plants ever brought together. This was amassed by Baron Benjamin Delessert (1773–1847).

William Barbey died in 1914, Casimir de Candolle in 1918, and in both cases their famous collections soon afterward became the property of the city of Geneva and were consolidated with the collections of the already existing city institution. Each of the great herbaria had required an equally extensive and important library for use in connection with it, and the consolidation resulted in much duplication. Some of the duplicates could be used, but it was decided to sell them in a single lot to some other botanical insti-

tution. The opportunity to acquire this collection was recognized by the director of the New York Botanical Garden, Dr. N. L. Britton, as one of the kind that comes but once in a lifetime. The offer was tentatively accepted by him at once and arrangements begun for acquiring the collection, which is now rapidly being incorporated into the library of the garden.

The collection of about 5,000 bound volumes, and still unaccounted thousands of pamphlets, is noteworthy in several respects. It contains many rare works, much used in botanical study, but difficult to secure. A large number of the books are in fine bindings, the majority of the bindings as good as new. There are complete sets of many periodicals, which form the backbone of any scientific library.

THE TORONTO MEETING OF THE BRITISH ASSOCIATION

THE preliminary program of the annual meeting of the British Association in 1924, to be held in Toronto, Ontario, on August 6–13, under the presidency of Sir David Bruce, has been issued and is abstracted in *Nature*. This will be the second occasion on which the association has visited Toronto, the first being in 1897, under the presidency of Sir John Evans, the second of the three meetings previously held in Canada (Montreal, 1884; Toronto, 1897; Winnipeg, 1909). Active measures are being taken, both in Toronto and at home, with the object of ensuring that the meeting shall afford an exceptional opportunity for intercourse between British, Canadian, American and European workers in science, and, to visiting members, a unique occasion for acquainting themselves with the manifold scientific interests of the Dominion. The University of Toronto, which ranks with its affiliated colleges as one of the largest in the British Empire, will be the principal center of the meeting.

The association will meet in thirteen sections as follows, the names of the president and recorder of each being given, together with the address of the latter: A (Mathematics and Physics): Sir William Bragg; Professor A. O. Rankine, Imperial College of Science and Technology, London, S.W. 7; B (Chemistry): Sir Robert Robertson; Professor C. H. Desch, University, Sheffield; C (Geology): Professor W. W. Watts; Professor W. T. Gordon, King's College, Strand, London, W.C. 2; D (Zoology): Professor G. Elliot Smith; Professor R. D. Laurie, University College, Aberystwyth; E (Geography): Professor J. W. Gregory; Dr. R. N. Rudmose Brown, University, Sheffield; F (Economic Science and Statistics): Sir William Ashley; Professor H. M. Hallsworth, Armstrong College, Newcastle-upon-Tyne; G (Engineering): Professor G. W. O. Howe; Professor F. C. Lea, 36 Mayfield Road, Moseley, Birmingham; H (Anthropology): Dr. F. C. Shrubsall; Mr. E. N. Falaize, Vinchelez, Chase Court Gardens, Enfield, Middlesex; I (Physiology): Dr. H. H. Dale, Professor C. Lovatt Evans, Physiological Laboratory, St. Bartholomew's Medical College, London, E.C. 1; J (Psychology): Professor W. McDougall; Dr. Ll. Wynn Jones, 7 St. Mary's Avenue, Harrogate; K (Botany): Professor V. H. Blackman; Mr. F. T. Brooks, 31 Tenison Avenue, Cambridge; L (Educational Science): Principal Ernest Barker; Mr. D. Berridge, 1 College Grounds, Malvern; M (Agriculture): Sir John Russell; Mr. C. G. T. Morison, School of Rural Economy, Oxford.

The inaugural general meeting will be held on Wednesday, August 6, in the Convocation Hall of the University of Toronto, when Sir David Bruce will deliver his presidential address. In the sections, addresses will be delivered by the respective sectional presidents, and papers will be read, on and after Thursday, August 7, until the conclusion of the meeting (Wednesday, August 13).

A preliminary program of excursions after the meeting is also being arranged. For those able to devote the maximum time, an excursion across Canada to Vancouver, and possibly also to Prince Rupert and Victoria, is contemplated.

CHARLES W. ELIOT

ON behalf of the Harvard Alumni Association its Secretary, J. W. D. Seymour, announces that in honor of the ninetieth birthday of Charles W. Eliot, president emeritus of Harvard University, a public tribute will be paid in Cambridge, Mass., on March 20. The event will be attended not only by representatives of the 45,000 Harvard Alumni, but also by leading citizens from all over the country who have shown their desire to honor the president emeritus in recognition of his services as "a citizen." A Citizens Committee is now in the process of organization; its membership, which will consist of many national figures, will be announced later.

Charles W. Eliot was president of Harvard University for forty years—from 1869 to 1909; he was born in Boston in 1834, and graduated from Harvard in 1853. His election as president was considered remarkable, both because of his youth—he was only 35—and because he was a layman and scientist.

While president, Mr. Eliot led in the development of graduate schools and the elective system in undergraduate work. The Harvard Medical School and the Harvard Law School largely grew to their present importance and influence under his administration.

And within recent years the Harvard Graduate School of Business Administration, founded in 1908, next to the last year of Mr. Eliot's term, has become one of the important professional schools.

The raising of the entrance requirements of universities, which has led to a corresponding raising of the standards of graduate schools, and the introduction of choice in entrance requirements, have had a nation-wide influence under President Eliot's leadership. As chairman of a committee of ten of the National Education Association in 1890, he exerted a strong influence on secondary education throughout the country.

In the course of his life President Eliot has received many honors from European governments. He is an officer of the Legion of Honor of France; a grand officer of the Crown of Italy; a member of the Imperial Order of the Rising Sun of Japan, and a member of the Order of the Crown of Belgium. He is also a member of the Moral and Political Science Academy of the French Institute; a member of the British Academy, a fellow of the American Academy of Arts and Sciences, a member of the American Philosophical Society, honorary president of the National Conservation Association, president of the Massachusetts Society of Social Hygiene, and past-president of the National Education Association and of the American Association for the Advancement of Science.

SCIENTIFIC NOTES AND NEWS

Services in memory of Jacques Loeb, who died suddenly in Bermuda on February 12, were held at the Rockefeller Institute for Medical Research on the afternoon of Sunday, February 17. Addresses were made by Dr. Simon Flexner, Professor W. J. V. Osterhout and Dr. William H. Welch.

In memory of George Lincoln Goodale, professor of botany at Harvard University from 1873 until his retirement as professor emeritus in 1909, half of a gift of $100,000 made to the university by an anonymous donor will be used to establish a George Lincoln Goodale Fund, the income of which is to be used to meet the current expenses of the Botanical Museum. The other half will be added to the permanent fund of the Arnold Aboretum, the income of which is also to be used for current expenses.

At the session of the Paris Academy of Sciences of January 2, M. Guillaume Bigourdan assumed the presidency for the year 1924, succeeding M. Haller.

The gold medal of the Royal Astronomical Society has been awarded to Professor A. S. Eddington, for his work on star-streaming, on the internal constitution of a star and on generalized relativity.

At the St. Louis meeting of the Federation of Biological Societies, Professor A. J. Carlson, of the University of Chicago, was elected president of the American Physiological Society; Dr. Philip A. Shaffer, of Washington University Medical School, president of the American Society of Biological Chemistry, and Dr. Aldred S. Warthin, professor of pathology in the University of Michigan, president of the American Pathological Society.

Dr. Felix Lagrange, professor of clinical ophthalmology in the University of Bordeaux, has been elected a correspondent of the Paris Academy of Sciences in the section of medicine and surgery.

Sir Charles Sherrington, Dr. J. H. Drysdale, Dr. F. J. Poynton, Dr. J. H. Abram and Professor T. Wardrop Griffith have been elected councillors of the Royal College of Physicians, London.

At the annual meeting of the New York Pathological Society, Dr. William H. Woglom was elected *president;* Dr. George L. Rohdenburg, *vice-president;* Dr. Francis Carter Wood, *treasurer,* and Dr. Leila C. Knox, *secretary.*

At the annual meeting of the New York Academy of Medicine the following chairmen were elected for the sections: ophthalmology, Dr. Ben Witt Key; orthopedic surgery, Dr. Sigmund Epstein; medicine, Dr. John H. Wyckoff, Jr.; genito-urinary surgery, Dr. Julius J. Valentine; historical medicine, Dr. Leon Pierce Clark.

E. F. W. Alexanderson, consulting engineer of the General Electric Company and chief consulting engineer of the Radio Corporation of America, has been awarded the Order of the Polonia Restituta by the Polish government in recognition of his services in connection with the building of Poland's new radio station near Warsaw.

Professor Heinrich Ries, of Cornell University, has been elected as representative of the Geological Society of America on the National Research Council.

The Messel medal of the Society of Chemical Industry has been awarded to Lord Leverhulme. The Messel medal is given to an eminent man distinguished either in chemical science or in chemical industry, who is asked to deliver the Messel Memorial Lecture at the annual meeting of the society. The first award was made in 1922 to Professor Henry E. Armstrong, F.R.S., who delivered his lecture at the annual meeting in Glasgow.

The council of the British Institution of Electrical Engineers has made the third award of the Faraday medal to Dr. S. Z. de Ferranti, a past president of the institution. The Faraday medal is awarded either for notable achievement in electrical engineering or for conspicuous service to the advancement of electrical science, without restriction as regards nationality.

The king of Italy has decorated a number of French physicians in honor of the Pasteur centenary, conferring the grand cross of the Order of the Crown on Emile Roux, director of the Pasteur Institute, and on M. Paul Strauss, minister of hygiene, while Drs. Chauffard, Pottevin and Honnorat, A. Calmette, L. Martin, Borrel, Roger, Achard, Salimbeni, F. Rathery and G. Poix were made officers of the order. Drs. Vincent, F. Widal, P. Carnot, V. Bérard, and René and Pasteur Vallery-Radot were made officers of the Order of Saints Maurice and Lazarus. The list includes the officers of the Pasteur institutes at Paris and Strasbourg, professors in the universities and the medical descendants of Pasteur and several senators.

Dr. George W. Crile, for twenty-three years professor of surgery, Western Reserve University Medical School, and house surgeon of the Lakeside Hospital, Cleveland, has resigned, effective on July 1. Dr. Crile will devote his time to research work and private practise at the Cleveland Clinic and Hospital. Dr. Eliot C. Cutler, Peter Bent Brigham Hospital, Boston, will succeed Dr. Crile in both positions.

Dr. Charles Sheard, Ph.D. (Princeton), has accepted the position of chief of the section of physics and bio-physical research at the Mayo Clinic, Rochester, Minn., and expects to take up the work early in March. He was the first professor and director of the work in applied optics at the Ohio State University. In 1919 he became connected with the American Optical Company as head of the department of ocular and professional interests and in general charge of many of the scientific activities of this company. He has also been the editor of the *American Journal of Physiological Optics* since its founding in 1920.

Dr. Emmett Carver has recently joined the staff of the Research Laboratory, Eastman Kodak Company. Dr. Carver is son of the well-known economist of Harvard University, and received his graduate and undergraduate training at Harvard. His graduate work was done under Professor Richards. During the war he held command as captain in the Intelligence Division, and for two years following a National Research Council fellowship. For the last three years he has been on the chemical faculty of the University of Illinois where he taught physical chemistry.

Dr. Robert D. Curtis, Medical School of Harvard University, Boston, has been appointed director of child hygiene for the Community Health Association and has been given a year's leave of absence from the Massachusetts General Hospital to carry on the work.

Dr. Enos H. Bigelow, president of the Massachusetts Medical Society, has been appointed director of the division of communicable diseases in the state health department.

Dr. W. V. Tower, entomologist of the Porto Rico Federal Station, has resigned to accept a position with the Porto Rican tobacco company.

Dr. W. A. Murrill recently sailed for Buenos Aires to visit the botanists of Argentina and secure botanical specimens for the herbarium of the New York Botanical Garden.

Sir Arthur Shipley, vice-chancellor of the University of Cambridge, chairman of the Imperial College of Tropical Agriculture in Trinidad, was a delegate to the West Indian Agricultural Conference meeting in Jamaica at the beginning of the month; he will subsequently visit the United States.

The program of the Philosophical Society of Washington on February 23 was: C. V. Hodgson: "Structural improvements in modern micrometer theodolites"; W. R. Gregg: "The relations between free air temperatures and wind direction."

Professor C. G. Barkla will deliver the seventh Silvanus Thompson Memorial Lecture of the Röntgen Society at the Institution of Electrical Engineers, London, on April 1.

At the annual meeting of the Royal Astronomical Society held on February 8, Sir Frank Dyson, Professor H. H. Turner and Mr. Evershed spoke on the progress of astronomy, and the president, Dr. J. L. E. Dreyer, gave a short address on the desirability of a new edition of Isaac Newton's works. After the annual general meeting Mr. F. W. Aston gave an address on isotopes.

Professor Henry Carmichael, consulting chemist, formerly professor of chemistry at Bowdoin College, died on January 29, at the age of seventy-seven years.

Professor Marcus Hartog, emeritus professor of zoology at University College, died at Cork on January 21.

Gustaf Eneström, of Stockholm, known for his work on the history of mathematics, has died at the age of seventy-one years.

Professor R. R. Thompson, professor of oil mining in the University of Birmingham, died on January 24, aged thirty-nine years.

William Mark Pybus, Esq., an eminent English lawyer who devoted his spare time to natural history, mainly to ornithology, died on January 4 at the age of seventy-two years.

M. Stephan, honorary director of the Marseilles Observatory and correspondent of the section of

astronomy of the Paris Academy of Sciences, has died at the age of eighty-seven years.

IN honor of the three hundred and sixtieth anniversary of the birth of Galileo, on February 15, 1564, the Boston Public Library has arranged an exhibition of the earliest and rarest editions of Galileo's works in the Barton room.

WE learn from *Nature* that arrangements are being made by a committee convened by the Royal Society to celebrate on June 26 the centenary of Lord Kelvin's birth. The committee is composed as follows: Sir Richard Glazebrook (chairman), Professor F. O. Bower (Royal Society of Edinburgh), Mr. W. R. Cooper (Physical Society), Sir John Dewrance (Institution of Mechanical Engineers), Mr. D. N. Dunlop (hon. secretary), Mr. F. Gill (Institution of Electrical Engineers), Sir Donald MacAlister (vice-chancellor of the University of Glasgow), Sir Charles Morgan (Institution of Civil Engineers), The Duke of Northumberland (Institution of Naval Architects), Dr. E. C. Pearce (vice-chancellor of the University of Cambridge), Dr. Alexander Russell (Institution of Electrical Engineers), Mr. F. E. Smith (Royal Society). A large number of Dominion, American and foreign men of science and engineers will be attending conferences at the British Empire Exhibition at that time, and July 10 and 11 have been selected as convenient dates for the Kelvin centenary celebrations. These will include a meeting for the receipt of addresses from delegates, at which Sir J. J. Thomson will deliver a memorial oration, and a dinner at which the Rt. Hon. Earl Balfour has promised to preside.

THE one hundred and twenty-fifth regular meeting of the American Physical Society will be held in Schermerhorn Hall, Columbia University, New York, on February 23.

WE learn from *Nature* that on January 23 Mr. W. B. Hardy formally declared open a laboratory for research in colloid chemistry and physics which has been founded in the University of Manchester by the generosity of a number of Lancashire firms. This laboratory, which has been named after Thomas Graham, the founder of the science of colloids, consists of two large and three small rooms, and is being specially equipped for the proposed course of research. It has been put in the charge of Mr. D. C. Henry, of Trinity College, Cambridge, who has been for two years a lecturer in chemistry at the University of Manchester.

THE International Congress of Mathematicians will hold its meetings at Toronto from August 6 to 13.

THE Fourth Congress of Industrial Chemistry under the auspices of the Société de Chimie Industrielle will be held at Bordeaux, France, from June 15 to 20. It is hoped that all of the American delegates to the International Union of Pure and Applied Chemistry at Copenhagen will be able to attend the meeting. American chemists desiring to present papers at this congress should send in a request for an application blank to Dr. J. E. Zanetti, Chairman, Division of Chemistry and Chemical Technology, National Research Council, Washington, D. C.

AT the medical congress held under the auspices of the British Medical Association in Melbourne opportunity was taken to open the new anatomy department of the University of Melbourne.

A SCHOOL of geographical surveying and field astronomy has recently been organized under the auspices of the American Geographical Society, which will provide the means whereby training may be obtained in the methods of accurate surveying, particularly for purposes of exploration.

THE trustees of Mount Sinai Hospital, New York City, have ratified the program for a $1,500,000 home and training school for nurses, and have themselves subscribed $735,000. A pledge of $200,000 was made by George Blumenthal, president of the hospital. The training school will occupy the south side of Ninety-ninth Street, opposite the hospital.

THE Natural History Museum, South Kensington, has been presented by Mrs. Wood, widow of the Reverend Theodore Wood, with a named collection of 14,000 specimens of British coleoptera, and 3,000 specimens of varieties from localities outside Britain. The collection includes a number of specimens which fill gaps in the study series in the Insect Room.

UNIVERSITY AND EDUCATIONAL NOTES

THE University of Naples, founded in 1224, will celebrate this year its seventh centennial.

DURING December of this last year, the National Southeastern University at Nanking, China, lost by fire its most important building which housed the library and the departments of agriculture, biology, education and engineering. The loss of the library was the greatest misfortune. Publications, especially those dealing with education and science, are urgently needed and gifts will be most welcome.

PLANS for the erection of a wing to the University of Pennsylvania Medical School to house the laboratories of anatomy and physiologic chemistry, have been submitted to the board of trustees by Dean William Pepper. This addition is made possible by the recent gift of the Rockefeller Foundation and the General Education Board, which insures the univer-

sity a building fund of $1,000,000 on condition that the university raises $500,000. The funds are already in hand. The new addition will be a T shaped wing immediately adjoining the present building on Hamilton Walk, permitting the university's laboratories of physiology, pathology, pharmacology, anatomy and physiologic chemistry to be together.

Dr. Joseph S. Ames, professor of physics, has been elected dean of the college faculty at the Johns Hopkins University beginning March 1, when the resignation of Professor John H. Latané becomes effective.

Dr. Victor E. Monnett has been appointed acting head of the department of geology of the University of Oklahoma to succeed Dr. J. B. Umpleby, who resigned recently to become vice-president of the Goldine Oil Company of New York.

Dr. Martinez Vargas, dean of the Barcelona faculty of medicine and a well-known pediatrist, has been nominated rector of the University of Barcelona.

Dr. William Campbell, bacteriologist of the city of Bradford, England, has accepted the Wernher Beit chair of bacteriology at the University of Capetown, South Africa, succeeding Dr. T. J. Mackie.

DISCUSSION AND CORRESPONDENCE

ELECTRICITY AND CHEMISTRY STUDENTS

The whole theory of transfer of electricity as it applies to chemistry is in a sad state of development so far as many of our text-books are concerned. The old dualistic notion of electricity is still sometimes retained in chemistry, while in physics the more modern conceptions are generally taught.

In the last edition of a widely used physical chemistry text-book the discussion of the Daniell cell reads: "When the zinc and copper electrodes are connected by a wire, a current of positive electricity passes from the copper to the zinc, along the wire." How much better if we would adopt the modern view and say that a stream of electrons flows along the wire from the zinc to the copper? This is just one example, while many others might be cited from books for elementary students. One author says: "The direction of the current as arbitrarily named is opposite to the flow of electrons along the wire. This decision as to the direction of flow was made before scientists knew anything about electrons." Why stick to a system of nomenclature which misrepresents the facts and confuses the student? Why not from the first explain the modern conception of matter? Whatever positive electricity is, it certainly never flows along a wire. It does, apparently, constitute the main mass of the "building stones," hydrogen and helium, but the ultimate composition of these masses is not well understood. These central cores of atoms certainly do not flow along wires. Why be so conservative about introducing the new conception of atoms? At present many of our best students come to our advanced courses with the idea that the ions carry electricity through an electrolyte, much as a gang of laborers would carry coal from the street to the furnace room. Why not from the beginning teach that the electrons which come from the battery never go to the "positive pole," and that the electrons supplied to the "positive pole" are those originally present on the negative ions of the electrolyte? The modern conception is no more difficult to grasp and it is at least nearer the truth.

Earl C. H. Davies

Morgantown, West Virginia

CHEMICAL SPELLING MATCH NO. 2

The idea of a chemical spelling bee, as explained by Professor Jacobson in Science of September 29, 1922, p. 368, made a strong appeal to us here and seemed to offer considerable help in part of the teaching work of freshman chemistry. We decided shortly before Christmas to hold such a contest and Dr. Hale, director of the department of chemistry, presented the matter to the seven sections of students taking this course. The vote was unanimous for adoption; from this time forth short preliminary matches were held in the various sections, score being held for each of these.

After several weeks the students seemed to tire somewhat and to lose interest in the contest. However word soon got about that a certain section was confident of winning the prize and from that time on competition and interest grew steadily. Students willingly and eagerly spent much extra time in drill, and pledged their instructors for extra drill periods.

The five in each section making highest scores in the preliminary drills took part in the final match. Each team was named and the members wore insignia. Graded lists of names and formulae were typed in triplicate and handed the judges and the reader. Preliminary to the contest several reels of moving pictures of the Production of Sulfur were shown.

Interest throughout the final contest was keen and decided. The prize offered was too small to have any effect of its own, and the entire interest was of a personal nature. Toward the close, when only four of the original thirty-five were left standing, excitement rose to a considerable pitch.

We adopted this contest because we hoped it would be the means of teaching the students valence, position of the elements in the periodic system, and something of the nature of compound formation. It has

done all these things in an interesting way, and has decidedly helped even those students who were well aware from the start that they could not place in the finals.

Several copies of the formula list are on hand, and may be secured by application to Dr. Hale.

E. Wertheim
College of Arts and Sciences,
Fayetteville, Ark.

A CASTIGATION AND AN APPEAL

Recently Professor G. A. Miller paused in a general address before the mathematicians of America to point out what he considered a flaw in one of my books. He said (this journal, 1924, p. 4) that it is "not true that he (Benjamin Peirce) was in charge of this almanac (the Nautical Almanac) for some years." Miller says that Peirce "did much work on the Nautical Almanac" and "was consulting astronomer from 1849 to 1867." The difference in content between Miller's statement of about fifteen words and my statement of three words ("in charge of") is the same as the difference between tweedledum and tweedledee. The admissibility of my phrasing is still more evident in the light of a passage in the preface of the first volume of the Nautical Almanac: "The theoretical department of the work has been placed under the special direction of Professor Benjamin Peirce, LL.D., and most of the calculations have passed under his final revision." It should be noted that in my book I carefully avoided saying that Peirce held the official title of "Superintendent"; such a statement would have been erroneous.

It should be noted that Professor Miller made two other errors in the passage which he devotes to my book. He informs the mathematicians of the country that among the men "in charge" of this almanac were "J. H. Coffin (1865–1877), and Simon Newcomb (1877–1894)." Now J. H. Coffin was a meteorologist and professor at Lafayette College, and was not superintendent of the Nautical Almanac; it was John Huntington Crane Coffin who was superintendent. Secondly, Newcomb did not retire in 1894. William Harkness, in his preface to the almanac dated September, 1897, says: "Professor Simon Newcomb, U. S. N., was director of the almanac until March 11, 1897." Miller's address contains some other misleading statements, but I confine myself to the part which relates to my book.

However, I do wish to make an appeal for fair play. Unfriendly critics are usually satisfied when they give a book one thorough overhauling. Not so Professor Miller. He prepared a long review of my book and then, during the past four years, followed it up with a procession of articles in various journals, further attacking that book. If his historical criticisms were careful and accurate, his course might be justified. But I am prepared to show that many of them are not. Many of them are partly or wholly wrong. Also, most of them are superficial in the sense that Miller does not usually consult the original sources. Is it too utopian an appeal to the spirit of fair play to propose that, when a critic finds that he himself is mistaken, he should do justice to the author criticized by publicly retracting his erroneous criticism?

Florian Cajori
University of California

QUOTATIONS

MOUNT EVEREST AGAIN

The time is drawing near when, for the third time in four years, the climbers and scientists of the expedition organized by the Royal Geographical Society and the Alpine Club for the ascent of Mount Everest will turn their faces towards Tibet. General Bruce, once again the leader, as he was in 1922, starts this week for India, with Major E. F. Norton, about a month in advance of the main body, to make the final preparations on the spot. In the article which we publish this morning he discusses some of the chief factors on which the chances of success must depend. The proved competence of the British *personnel* and of the hillmen who acted as porters in 1922—impervious, he says, to cold, exposure and fatigue—is, to begin with, an asset of the highest value. Other assets are the insight which has been gained into the character of the people with whom the expedition will have to deal, and the extreme friendliness of the existing relations between the British and the government of the Dalai Lama—mainly brought about, General Bruce, says, by the action of our own political officers. It may be added that the leader's own wide knowledge and understanding of the Himalayan races have been, and will be again, of great service in the conduct of the expedition. General Bruce considers that the experience in high acclimatization gained during the last expedition will enable the use of oxygen to be delayed till a much later stage than had hitherto been considered practicable, and that this economy in its use will tend to simplify the difficult problem of transport on the highest slopes. On the other hand, the one unknown and unknowable factor—in itself the most important of all—will be the weather. Last year the monsoon, and with it the adverse conditions which cut short the time available for the one last and most promising attempt to reach the summit in 1922, was delayed. If, by great good fortune, the same thing happens this summer, the chances are probably in favor of success. If not—if, that is to say, the bad weather again comes before its time—

they will be seriously lessened. The ladder of camps essential for the final assault can only be established rung by rung, a process that must take time, and can only be continued while the favorable conditions last. But whether the result be victory or defeat, the third attempt to conquer Everest will mean, like the two before it, an inspiring display of the resolution and endurance and indifference to discomfort and danger that, all through the ages and to the uttermost ends of the earth, have made the people of these islands, above all things, a race of pioneers. When General Bruce says that the great adventure of Everest has now almost become a pilgrimage, he touches upon a profound truth. Just because the way is long and difficult and beset with dangers, the mere attempt to progress along and up it is worth while—worth while not only for the pilgrims who first try to set foot on these untrodden peaks, but for the help of all those after them who will try to do hard things, because hard things are an end in themselves, even with the possibility of failure and no material gain in view.—*The London Times.*

SCIENTIFIC BOOKS

The Coming of Man. By JOHN M. TYLER. Marshall Jones Company, Boston, 1923, pp. 142.

THERE is a real need and ought to be and probably is a large field for such a book as the one before us. This is a day of greatly quickened interest in questions involving the nature, the origin and the destiny of man. And certainly these questions are now asked on the basis of more general enlightenment and with more vital interest than ever before.

One evidence and one consequence of this is an increasing number of inquiries for reading matter on the subject by persons of general education. No one whose professional status is presumed to be a guarantee of his competency escapes requests for advice concerning such matters. It would be a great satisfaction to those solicited could they recommend without misgivings the reading of some one or a few books.

The Coming of Man is designed, the author tells us in his preface, to meet the needs of the class of persons here indicated. And on the whole the design seems to the reviewer pretty well realized. Indeed in most respects it seems to him so superior to the usual run of what is now being written in this general domain that for the present it will stand first in the list of books he can recommend to such inquirers. For one thing in particular the author deserves commendation: He has largely avoided the dogmatic speculation that puts much of later writings of this nature outside the possibility of commendation by a conscientious adviser.

As might be expected from a life-long teacher of biology, the biological groundwork for the origin of man is quantitatively ample. Indeed one may justifiably question whether it is not more than ample, for nearly a third of the volume is devoted to it. In the reviewer's opinion some of the space given to such subjects as the skeletal parts of arthropods and molluscs could more profitably have been devoted to subjects that come closer home to human beings but which are given scant attention. Examples of such subjects are myth and superstition. A chapter on the "Nature of Man" in a book on human development which contains no reference to these cardinal matters strikes one even moderately informed about the lives of primitive people, as the play of Hamlet with the character of Hamlet left out, sure enough.

The reviewer is truly desirous that his full judgment of the book shall be accepted as favorable. But he believes his expression of that judgment can be most effective if it follows upon the heels of a reference to what seems to him a serious defect in it. And the criticism just made points toward that defect.

So great has been the progress in recent years of man's knowledge of his own mental life, and so inextricably is that life now known to be interwoven with his whole life, that the day is forever gone when any book, however small, on human evolution, can be counted as modern and adequate which does not give as much space to mental as to physical development.

Yet the author of *The Coming of Man* gives his readers distinctly to understand that if they wish information about the coming of this part of their nature they must go elsewhere to get it. "The whole subject" we read (p. 26) "of instinct and intelligence, their resemblances and differences, compensating advantages and disadvantages, especially their origin, forms a field of most fascinating study, *into which we will not attempt to enter*" (italics by the reviewer).

This I submit is almost tantalizing. It seems to say to the reader, "Although I know a good deal about these matters in which you are undoubtedly much interested, I refuse to tell it to you."

Beyond a question the most distinctive thing, and the most important thing about man is his mind. And, as already intimated, so aware of this is general enlightenment, and so eager is it for more enlightenment concerning many aspects of mind, that it seems inevitable that in the near future authors who contemplate writing general books on human evolution but are unwilling or unable to include the evolution of mind in their undertaking, will on second thought see that it would be better to leave the whole job to somebody else.

But the book before us has in this a saving grace. The truth is, its author does not do as badly as he promises to. For in reality there are many enterings

(even though for only a little way) into matters psychological scattered through several of the chapters. And in this, as in several other respects, the book is a distinct improvement on many another production of recent date in the same general field. For example, the chapters entitled "The Dawn of Civilization" and "The Rise of Personality" are exhilarating promises of how vast and how vital the drama of human life upon the earth will be seen to be, even by persons of ordinary education, when once a few biologists competent to tell well the material side of the story shall have become sufficiently educated to tell its spiritual side also.

WM. E. RITTER

LABORATORY APPARATUS AND METHODS

AN EFFECTIVE ABSORPTION APPARATUS[1]

DURING an investigation which aimed to determine qualitatively and quantitatively the gaseous evolutions from flowers of sulphur and from ground sulphur when freely exposed to the atmosphere and to bright sunlight different types of absorption apparatus were tried out in an endeavor to find one suitable for the purpose to which it was to be applied. Owing to the fact that the gases to be absorbed were very small in quantity and distributed through relatively large volumes of air it was quite essential that the absorbing apparatus should be efficient and capable of continuous operation for a considerable period of time. In examining the various types of apparatus such points as efficiency, compactness, rigidity, ease of sampling, ease of refilling, and cleaning were considered. To meet the particular experimental requirements it was necessary to devise and construct a special type of apparatus.

The essential features of the apparatus which was finally adopted and constructed are shown in longitudinal section in figure 1. The apparatus consists of a heavy walled bacteriological culture tube A about 25 mm. outside diameter and 150 mm. long to which is sealed near its lower end a side arm tube B 4 mm, in diameter. The lower end of the side arm tube is drawn out to a narrow tip which is directed downward toward the bottom of the tube A. The opening through this tip is quite small to permit the formation of only very small bubbles coming at regular intervals when the apparatus is in operation, the bubbles being released near the side wall at the bottom of the tube A.

The central tube E is about 12 mm. outside diameter and 125 mm. long around which 60 cm. or

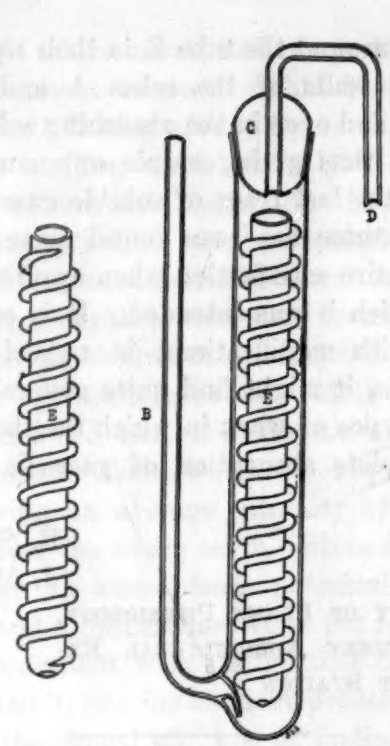

FIG. 1

more of 4 mm. glass tubing is wound to form the spiral which is sealed off at both ends to prevent the entrance of liquids. The central tube E is open at both ends the opening at the upper end having the full diameter of the tube while that at the lower end is somewhat smaller; the tube, being rounded at this end, serves as a guide to direct the escaping bubbles against the spiral. The spiral with the central tube E slides freely into the tube A but fits snugly enough to prevent the bubbles from escaping between the spiral and the walls of the tube A. The spiral with the tube upon which it is wound, being movable, can be adjusted in the tube A in any position with respect to the tip of the side arm upon which it rests. It can be removed readily from the tube A for the purpose of cleaning which is an important matter to be considered.

The tube A is closed with an air tight seal by means of the ground glass stopper C which holds the outlet tube D. A rubber stopper holding the outlet tube may be substituted, of course, for the ground glass stopper if experimental conditions permit.

In operating the apparatus the tube A containing the spiral and central tube E is partially filled with the absorbing liquid after which the stopper holding the outlet tube D is put in place. The apparatus is then adjusted to receive the air containing the gases to be absorbed, these entering through the tube B when suction is applied at D. Suction may be applied continuously by the use of an ordinary small aspirating pump attached to a constant level reservoir to insure steady action.

Air and the gases to be absorbed, entering the apparatus, are delivered into the absorbing solution at the lower tip of the side arm tube B in the form of small bubbles which are caught by the spiral, passed

[1] Paper No. 140 of the Journal Series, New Jersey Agricultural Experiment Station, Department of Plant Physiology.

many times around the tube E in their upward course between the walls of the tubes A and E and are turned over and over in the absorbing solution during the process, thus giving ample opportunity for the removal of the last trace of soluble gases.

This apparatus has been found quite efficient and has given entire satisfaction when applied to the purpose for which it was intended. It is entirely probable that with modifications, in regard to size and minor details, it might find quite general application in various types of work in which the thorough washing or complete absorption of gases is an essential feature.

E. S. Stinson
J. W. Shive

Laboratory of Plant Physiology,
New Jersey Agricultural Experiment Station

SPECIAL ARTICLES

AIR-EARTH CURRENTS AND OTHERS

In Science of July 27, 1923, pp. 67–68, Dr. L. A. Bauer has criticised my article in Science of May 25 on the ground that my data was insufficient. I can only reply that I used the data upon which practically all the generalizations upon atmospheric potential gradient have been made. My Figure 1 represented the data from *Observatorio del Ebro* for the six years, 1914–1919, and seems to me to contradict Dr. Bauer's statement that the annual variation of atmospheric potential gradient "does not vary according to the sine of the sun's zenith distance at apparent noon at any given place."

In general, the annual variation which I described in my paper applies to the published data for the following stations besides Tortosa, as may be seen from the table on page 889 of Arrhenius's *Lehrbuch der kosmischen Physik:* Brussels, Kreuznach, St. Louis, Melbourne, Moncalieri, Paris, Ghent, Wolfenbüttel, Helsingfors, Sonnblick, Batavia, Kief and Stuttgart. Arrhenius says that the annual variation at Cape Horn, like that at Melbourne, is opposite to that in the Northern Hemisphere, but I have not seen a tabulation of Cape Horn data. Neither have I seen any data from Helwan, Egypt.[1a]

[1a] In his article on "Atmospheric Electricity," in the Dictionary of Applied Physics, C. T. R. Wilson says: "Such evidence as is available goes to show that the annual variation is of the same character with a maximum in midwinter and a minimum in midsummer throughout middle latitudes in both hemispheres, *i.e.*, everywhere outside the tropics and the polar regions. The records of potential obtained at Helwan (Egypt) are exceptional, showing a maximum in midsummer and a minimum in midwinter."

Also, while Dr. Bauer says the average annual variation of potential gradient varies by but 60 per cent., Arrhenius[1] says the average potential gradient over Europe is about 4.6 times as great in winter as in summer. The same is true for the St. Louis data.

In the article which Dr. Bauer criticized, I called attention to the importance of correcting the observed potential gradient for the conductivity of the air at the time of observation. This was done in the case of the data from Ebro Observatory, but no data on atmospheric conductivity was available for other stations. It was mentioned that this corrected potential gradient is what has usually been defined as an air-earth current. This current, as computed from the atmospheric potential gradient and the atmospheric conductivity amounts, according to Dr. Bauer's estimate,[2] to about 3×10^{-6} ampere per sq. km over the whole earth. This would give a total current of about 1500 amperes continually flowing into the earth, which would raise the electric potential of the earth at the rate of 2,400,000 volts per second. This change in potential, impossible as it seems, fades into insignificance in comparison with that which would accompany some of Dr. Bauer's hypothetical air-earth currents.

My paper of May 25 dealt only with the seasonal variation of atmospheric potential gradient; but there is likewise a diurnal variation of atmospheric potential gradient which must, if the phenomenon is one of induction by a charged earth, vary with the diurnal variation of the earth's potential at the place of observation. For the purpose of showing that such a relation is indicated, I am fortunately able to refer to data which have already been approved by Dr. Bauer. In *Terrestrial Magnetism,* XXV, page 161, Dr. Bauer gives two curves which show what he calls the summer diurnal variation of air-earth current density. One of these curves is there said to represent Dr. Dorno's observations at the Alpine station Davos, and the other is said to represent observations made at the Potsdam Observatory. In Figure 1, below, Dr. Bauer's Davos curve, as scaled from the one in *Terrestrial Magnetism,* is compared with a curve showing the diurnal variation of the electrical potential of the earth at my Palo Alto observatory for the year August, 1920–July, 1921, and in Figure 2, the Potsdam curve is compared with the curve of diurnal variation of earth potential at Palo Alto for the year September, 1921 to August, 1922. In both figures the broken line represents the Palo Alto curves. It will be seen that the air-earth current curves agree with the curves of earth potential variation as closely as they do with each other.

[1] "Kosmische Physik," p. 888.
[2] *Terr. Mag.*, XXV, 156 (Dec., 1920).

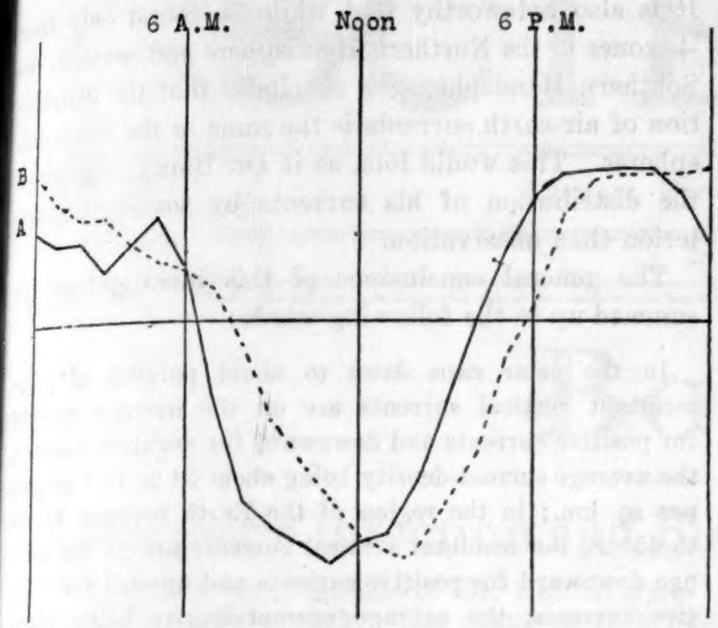

FIGURE 1.—Comparison of diurnal variation of air-earth current at Davos and of the Earth's potential variation at Palo Alto.
A, Air-earth current.
B, Earth-potential variation.

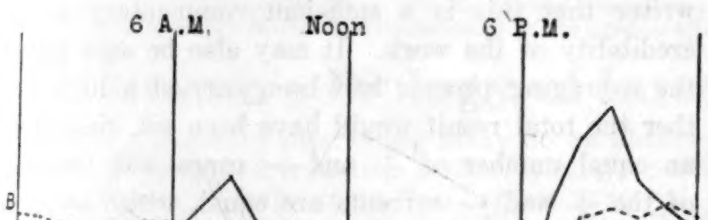

FIGURE 2.—Comparison of diurnal variation of air-earth current at Potsdam and of the Earth's diurnal potential variation at Palo Alto.
A, Air-earth current.
B, Earth-potential variation.

It is true, as Dr. Bauer will probably say, that neither pair of these curves was made at the same time or place; but Dr. Bauer has used the air-earth current curves for comparison with what he calls the diurnal variation of vertical current density of terrestrial magnetism for Cheltenham, Maryland.

And here seems an appropriate place to discuss briefly these vertical currents of terrestrial magnetism which have occupied so conspicuous a place in Dr. Bauer's writings and speeches for the past twenty years.

There have been numerous attempts to analyze the magnetic field of the earth, and to specify a distribution of permanent magnets within the earth or of electric currents flowing around the earth, either above or below its surface, which would give the observed distribution of magnetic force over the earth. None of these attempts has been completely successful, and some of the mathematicians have suggested that the variations from their computed distribution might be due to vertical electric currents flowing into or out of the earth over certain areas. Adolf Schmidt, for example, concluded that to make his analysis agree with observations there would be required vertical currents having an average intensity of 1/6 ampere per sq. km over the whole earth. Since such currents would require an atmospheric potential gradient of some seven and a half million volts per meter, as well as being inconsistent with many other observed phenomena, Schmidt, like his eminent predecessor, Gauss, apparently abandoned all hope of finding an alibi in air-earth currents.[3] Rücker, in England, made a careful computation of the line integrals of magnetic force around areas in the British Isles, where very accurate magnetic data were available, and found no vertical currents flowing into or out of the earth.

In 1904, Dr. Bauer hit upon an original method of locating these hypothetical air-earth currents from magnetic declination charts alone. As a fundamental proposition upon which to base his method, he declares[4] *"A downward electric current, i.e., one passing from the air through the Earth's surface, in accordance with Ampere's rule, will deflect the north end of a magnetic needle to the West."*

Now, as any one who has given any attention to the laws of the deflection of magnets by currents knows, this statement is wholly inconsistent with the facts of observation. In the first place, Ampère's rule says nothing about the points of the compass to which a magnet pole will be deflected by a current. As generally stated, it says that if an observer will regard himself as part of the conductor carrying the current and is so placed that the current will enter at his feet and leave at his head, then a magnetic needle *outside the current* and in front of him will have its north-seeking pole deflected toward his left hand, *no matter in what direction he faces.* In other words, the north-seeking pole of a magnet is driven around a current in clockwise direction as seen by one looking along the current in the direction it is assumed to be flowing. Hence the influence of a vertical current upon a horizontal magnetic needle is indeterminate until the relative positions of the current and magnet are specified.

[3] See Bauer, *Terr. Mag.*, XXV, 146.

[4] *Terr. Mag.*, IX, 123 (Sept., 1904).

A positive current flowing into the earth between the poles of a magnetic needle already oriented by the earth's field would tend to deflect its north end toward the *East,* instead of to the West, as Dr. Bauer's rule states.

Since announcing his discovery, Dr. Bauer has four or five times computed the distribution of vertical electric currents over the earth. In his paper entitled *On Vertical Electric Currents And The Relation Between Terrestrial Magnetism And Atmospheric Electricity,* in *Terrestrial Magnetism,* XXV, pp. 145–162 (Dec., 1920), Dr. Bauer again reiterates his rule of 1904, *viz., "A positive electric current passing, for example, from the air through the Earth's surface, in accordance with Ampere's rule, will deflect the north end of a magnetic needle to the West, a reversed current, on the other hand, would deflect the needle to the East."* Dr. Bauer then computes the line integral of magnetic force around various parallels of latitude, using for his points of computation values scaled from magnetic charts, and not actually measured at the points selected, and then calculates the total vertical current which is flowing into or out of the earth between his selected parallel and the pole, meanwhile apparently forgetting or ignoring the possible currents flowing into or out of the rest of the earth. He then divides the earth into zones of five degrees in width, and gives the total current and the current density for each zone. His conclusions regarding the current density for the different zones are shown in the following table, which is taken from his Table I, p. 151 of his article. The current density is given in amperes per square kilometer. A + sign indicates a current flowing from the earth into the air, *i.e.,* a negative potential gradient over the earth, while the — sign indicates a current flowing into the earth, and consequently a positive atmospheric potential gradient.

Zone	N. Hemisphere	S. Hemisphere
Pole to lat. 50°	+.021	+.018
Lat. 50° to lat. 45°	+.045	+.034
" 45° " " 40°	—.006	—.047
" 40° " " 35°	+.018	—.022
" 35° " " 30°	+.008	—.031
" 30° " " 25°	—.029	+.019
" 25° " " 20°	—.037	+.058
" 20° " " 15°	—.019	+.041
" 15° " " 10°	—.033	—.041
" 10° " " 5°	—.053	+.014
" 5° " Equator	—.018	+.009

After deciding upon the magnitudes of the currents in the different zones, Dr. Bauer proceeds to combine the results for corresponding zones north and south of the Equator, although in seven of the nine pairs of zones thus combined the currents in the two zones are of *opposite* signs. This seems to indicate that Dr. Bauer is not even certain in which direction the currents whose magnitudes he gives are actually flowing. It is also noteworthy that while he found only four + zones in the Northern Hemisphere and seven in the Southern Hemisphere, he concludes that the distribution of air-earth currents is the same in the two hemispheres. This would look as if Dr. Bauer determined the distribution of his currents by some other criterion than observation.

The general conclusions of this investigation are summed up in the following words:

> In the polar caps down to about parallel 45°, the resultant vertical currents are on the average upward for positive currents and downward for negative currents, the average current-density being about 24×10^{-3} ampere per sq. km.; in the region of the Earth between 45° N to 45° S, the resultant vertical currents are on the average downward for positive currents and upward for negative currents, the average current-density being about 10×10^{-3} ampere per sq. km.

It will be seen that in drawing this conclusion Dr. Bauer has reversed the observed signs of more than one third of his currents. It seems to the present writer that this is a sufficient commentary on the credibility of the work. It may also be seen that if the averaging process had been carried a little further the total result would have been *nil,* since there an equal number of + and — zones, and the sums of the + and — currents are equal, which seems to show that in observations of this character the law of probabilities still holds.

While it seems impossible to attach much probability to the existence of these currents, it is not without interest to see what would be some of their consequences if they did exist. Since they are, on the average, ten thousand times as strong as the currents deduced from the atmospheric potential gradient and the atmospheric conductivity, they would seem to require a potential gradient ten thousand times as high as observations have shown, that is, more than a million volts per meter. Also, in the regions between latitude 45° and the poles this potential gradient would be in the opposite direction to that which has been observed. On opposite sides of the parallels of 45° there would accordingly be a difference in the atmospheric potential gradients of from two to three million volts per meter. The total current flowing across these parallels would be 3,540,000 amperes. In the Northern Hemisphere the earth-currents would be from south to north, which is opposite to their observed direction.

But it seems needless to discuss these currents further until Dr. Bauer derives them, as he suggests in his last SCIENCE article that he may do, from the motion of the charged earth through the ether.

FERNANDO SANFORD

PALO ALTO, CAL.